

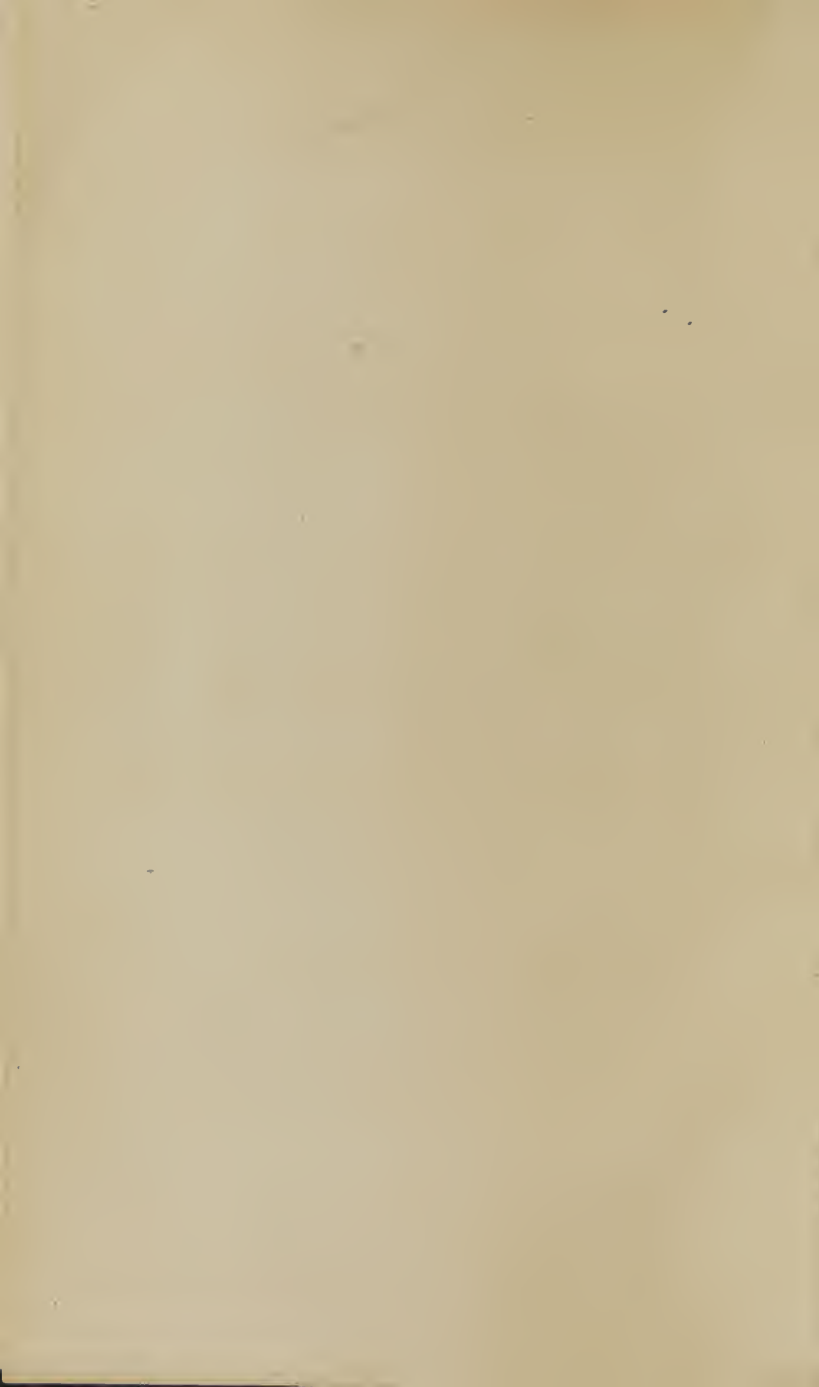
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BY WILLIAM ALLSTON,  
O F G E O R G E - T O W N, S O U T H - C A R O L I N A,  
*Member of the Philadelphia Medical and Chemical Societies:*

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P H I L A D E L P H I A:

P R I N T E D B Y W I L L I A M W. W O O D W A R D, N o. 173  
C H E S N U T - S T R E E T.



TO

*Robert Brownfield, A. M. Physician,*

GEORGETOWN, SOUTH-CAROLINA.

MUCH RESPECTED SIR,

*ACCEPT as a tribute of gratitude and esteem, the first fruits of that study which commenced under your patronage. And be assured, that a lively recollection of the many favors received from you, can only be terminated with the life of,*

*Respected Sir,*

*Your very sincere friend,*

*And ever affectionate pupil,*

WILLIAM ALLSTON.

May 3d, 1797.



T O  
BENJAMIN RUSH, M. D.

PROFESSOR OF THE  
Institutes and Clinical Medicine,  
IN THE  
UNIVERSITY OF PENNSYLVANIA.

HONORED SIR,

*TO* whom with more propriety can the following Dissertation be dedicated, than to him whose principles can be traced in every page of it?—Receive then this my first attempt, as a mark of that respect I owe to your illustrious character.

*Allow me, Sir, to make you my sincere acknowledgments for the invaluable discoveries and improvements you have made in medicine. And suffer me in this manner to present to you my unfeigned and most cordial thanks for your constant politeness and friendship, ever since I have had the honour of residing with your amiable family. And believe me, when I assure you, Sir, that no possible change of my situation can ever lessen that real affection which is now so fervent in the bosom of,*

*My honoured Sir,*

*Your affectionate friend,*

*And grateful pupil,*

WILLIAM ALLSTON.

May 3d, 1797.

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# D R O P S Y.

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WHEN the catalogue of diseases is examined, it will be evident, that of all which attack life, no one appears to be more constantly fatal in its termination than that which is called DROPSY. Sensible of this, the Author of the following DISSERTATION, having no other motive than a compliance with the laws of this University, and an earnest desire to alleviate the sufferings of his fellow-creatures, hopes it will escape the eye of criticism, and that every allowance will be made for this incorrect and youthful performance.

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## D E F I N I T I O N.

DROPSY is an appellation given to that disease in which there seems to be a preternatural collection of serum or water, in some one or more parts of the human body. A doubt has arisen respecting the derivation of this name: Some have supposed it from the Greek, while others\* have asserted it

\* Wilkes, page 2.

to come from the Saxon. As the present intention is not to dive deeply into the minutiae of this disease, but only to examine the general nature of it; and as such a disquisition would at best be but speculative, and could not in the least elucidate or advance the cure, I will leave this determination for future enquirers; conscious that although they may make it even of a different origin, they will all mean by it the same disease, produced by similar causes, and whose method of cure will be still the same.

THIS disease is said to be generally known by an enlargement of some part of the body, accompanied by particular symptoms, which are, a difficult respiration, great thirst, scantiness of urine, and swelled ancles. Others are said to occur frequently, though not constantly; as, a pain in the thorax, cough. As the four former are said always to be not only present, but strictly Pathognomonic, it cannot be amiss here to examine if this be really the case; if it be not, then the impropriety must appear of placing confidence in any symptom as being characteristic of one disease alone.—And first of

### DIFFICULT RESPIRATION.

THIS has long ago been considered as a symptom inseparable from this disease; Dr. Sydenham\*

\* Wallis's, page 262.

ranked it as a constant one. With all the deference due his great character, I must beg leave here to dissent from him in opinion; for although it may, and very frequently does exist, yet it cannot be imagined always to be present; for it is now very certain, that there never was a symptom which constantly accompanied any disease. The yellow fever, for instance, was supposed by many to have invariably that tinge from which it took its name; yet Drs. Mosely and Rush, and the too fatal experience of 1793, have taught the contrary. The error of Drs. Sydenham, Monro\* and others, must have been the result of a false theory, supposing it the effect of the pressure of the water upon the diaphragm, and the contents of the abdomen. This opinion must at once crumble to its original state, when from experience it has been found that this same symptom not only existed, but was equally as distressing in those diseases in which, from dissections, there was not discovered the smallest quantity of water; but on the contrary, evident marks of inflammation; as in those who have died from a pleurisy, the measles, or small-pox. Now surely no one will advance, that this symptom is characteristic of either of these diseases, as distinct from the others; for it exists in all: And if it is not, why should it be supposed so in the Dropsy? I must confess I know not. But this impropriety will be still more glaring when it is dis-

\* Monro, page 9.

covered, that it does not always take place in this disease ; and in proof of this, we are assured by Dr. D. Monro\* that his experience taught him there had been many dropfies in which this symptom did not exist. And if this symptom does not exist at all times in this disease, and is as often in other diseases, now supposed essentially different therefrom, might it not be accounted as much pathognomonic of the latter as of the former ? And if it be a symptom so common in most states of fever as experienced authors have recorded, and in all diseases primarily of the blood vessels, may it not, even in the dropfy, be attributed to an irregular or convulsive action of the arterial system ?

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### T H I R S T,

ON the next symptom for our present consideration, and which Dr. Sydenham† declares is one of the three absolutely necessary to constitute this disease. It cannot be denied, that this symptom attends as commonly, and is as distressing as any which occurs : but nevertheless, that it should here be considered as pathognomonic is not altogether so admissible ; for it is very common in every state of fever as well as in the Dropfy. But that it cannot be characteristic of this disease, it has ac-

\* Monro, page 9.

† Wallis's, page 262, vol. ii.

cording to Dr. Darwin\* been found wanting. This fact makes it, from the great analogy with the other states of fever, very probable; for in them there are states, as experience must have witnessed, even of the most inflammatory grade, in which this familiar symptom is frequently wanting. And if it be in this disease as in fever, may it not be doubted whether it be pathognomonic of the former; but rather, as in the latter, the entire effect of a morbid action of the blood vessels?

### DIMINISHED SECRETION OF URINE

Is the third and next pathognomonic symptom which has been noticed from the earliest ages of medicine to the present period. Vanhelmont, as absurd in this disease as in many others, assures us, that no man whose urine passes freely, will ever become dropical; and to this day it is a common direction, to observe its quantity. So far however from this assertion being grounded upon experience, Wallis,† Wilkes, and others affirm, there have been cases of this disease, in which no such symptom existed. And Dr. Leigh says‡ there have been dropical patients who, through the whole course of the disease, had their urine both in quantity and quality. That it is proper to pay attention to this discharge when we administer re-

\* Zoonomia, page 314, vol. i.

† In his translation of Sydenham, page 262, vol. ii.

‡ Nat. Hist. Cheshire, page 69.

medies to promote it, no one will deny ; but that it should be so narrowly watched, and even supposed the cause of this disease, must be the remains of ignorance and superstition. As well might we imagine the vomiting in a cholera morbus, or the discharge of <sup>c</sup>lybæa in the dysentery the causes of those diseases, when in reality they are an effect. Besides, this symptom has been found in the different states of fever, as in the eruptive; for Sydenham long ago in the small-pox cured this symptom only by exposing his patient to the cold air: and in the bilious remittent state Dr. Rush informs us in his account of that fever in 1793, that he often met with it.—From what has been advanced on this subject, I cannot by any means admit this symptom as pathognomonic of the Dropsy more than of the other states, but shall dismiss it with asserting it to be, as in the other states of fever, the effect of an irregular action of the arterial system.

### SWELLED ANCLES.

THAT swelled ancles have ever been considered as a symptom characteristic of this disease, all ancient authors will evidently prove; and Dr. Sydenham\* places it as an infallible one. From this sanction of antiquity, modern physicians scarcely ever once lose sight of it, and seldom or never do they attempt to prescribe for hydropic patients, without first enquiring if this exist, and if so, then the di-

\* Page 262, vol. ii.

fease is evident, and their judgment at once confirmed. But notwithstanding the universal prevalence of this opinion, I hope to be excused for not subscribing to it ; and that too, because from the writings of Dr. Sydenham\* it appears that he himself found it erroneous : hence shortly after he adds, “ it does not always hold good, for it is often in pregnancy, and frequently in obstructed menses. It is not only an uncertain one in women, but also in men, being often in them the consequence of asthma.” Here it must be certain the Doctor found this symptom in diseases which his principles could not at any rate admit as similar : how therefore could it prevail in diseases so widely different, and yet be pathognomonic of the one and not of the other?—This difficulty will be surmounted when it be proved that Dropsy is as much a disease of the arterial system as either obstructed catamenia or the asthma ; for then it will be plain, that in either case it is the action of similar causes upon the same system. But, that in all probability it is not a pathognomonic symptom, Dr. Wilkes† positively asserts, that there have been instances of this disease which terminated fatally, and in which the legs never once swelled. If this symptom also be not at all times connected with the dropsy, and exist in most states of fever ; have we better authority to make it characteristic of the one than of the other ? And if there be not, may I not conclude, that in every

\* Page 263, vol. ii.

† Page 44.

case, whether arising in dropfy, obstructed catamenia, scarlatina, or what it may, it is always the effect of an irregular or convulsive action of the arterial system?—It may here be asked, if I admit any of these symptoms as pathognomonic? The answer is in the negative; for I have already proved that they are all exceptionable, being absent in some dropfies and present in other diseases which very few or none will acknowledge of a similar nature. The utility of a proper theory, however ridiculed by many, can never perhaps appear in a more striking and satisfactory point of view than in the present disease; for when we admit dropfy to be a genuine fever, as I hope to prove, we at once destroy those innumerable contradictions, and every symptom is seen in its original simplicity, and must instantly be acknowledged the effect of the same cause. Then it will be readily admitted, that a difficult respiration is the effect of that irregular action of the vessels by which means the blood is propelled through the lungs with such impetuosity as to give rise to laborious breathing. Neither is this action always of an equal strength, but must alternate according to the quality and duration of the exciting cause; and hence the variety in this respiration. Thirst and paucity of urine may also be explained by admitting, that from this morbid action the blood is hastened in larger quantities than usual into the different glands; in consequence of which they are so choaked that the secretory vessels cannot perform their office. And this is in-

deed probable, because the kidneys have often upon dissections been found to have laboured under an engorgement ; and because cold and the common evacuants are effectual in restoring these secretions. Nor will this action of the vessels forsake me, when I examine the swelled ancles ; for it has already been urged, that an effusion is the entire effect of it ; causing the exhalants to throw out more than was natural. In doing which, from their excessive force the action of the blood vessels, if I may be allowed the expression, is raised or increased from the healthy or natural point\*, to the effusive. In slighter cases,

\* It must here be noticed, that the institution of these points promises the greatest utility to the world, and for which it must ever be indebted to the Professor of the Institutes of Medicine in this University. When a person is in good health he is at the *healthy point*, the first grade above which is the *point of simple morbid action* ; this is the first change from common health, and in it the arteries are alone diseased without the least local affection. When this action is more violent a congestion in some part takes place, and hence a pain ; this may be called the *point of congestion*, or rather, the *inflammatory point* : and if the system is raised higher, an effusion is either the consequence, and hence the *effusive point*, or a suppuration, which forms the *suppurative point*. And if the action of these vessels is yet increased to an higher pitch, a mortification takes place, and hence the *gangrenous point*. And either of these may exist without the others : for it has often been noticed that fevers have prevailed without any other affection than a simple morbid action of the blood vessels, while other states of it have an inflammatory affection from the commencement. Some have two or more without the others, as rheumatism the inflammatory and effusive points without the suppurative, and the small-pox the inflammatory and suppurative without the effusive ; and mortification

as of obstructed catamenia, or gravitation, in consequence of predisposing debility from much walking, the effusion is thrown altogether upon the lower extremities; but in those more violent, from the intestines being a more delicate part, the effusion is formed altogether in the abdomen; and in the most severe, every part of the whole body is liable to it. —If such then be the statement of the proper nature of these symptoms, they cannot be viewed in any other light than as the effect of an irregular action of the arterial system, which may at all times be discovered by a proper attention to the pulse or state of the system.

## D I A G N O S I S.

AS the history of medicine abounds with innumerable melancholy instances of pregnant women being by the most skillful not only imagined dropical, but have even been operated upon; and as many who really laboured under that disease, have been suffered to linger in torture; it must readily be acknowledged, that all these boasted criterions are but fallacious, and that the Diagnosis has at all times been uncertain. Under this head I shall only observe, that when this disease is considered as a fever, it will always be known by the irregular action of the arterial system.

the inflammatory and gangrenous without the effusive: And frequently in dropy the effusive exists without either the inflammatory or the gangrenous.

As the

## PROGNOSIS

Of a disease is often of the greatest consequence, not only to remove the apprehensions of the friends, but also to secure the character of a physician; it would at all times be an object to obtain such information as would enable an immediate and favourable prediction. In this disease however, as nothing certain has ever been discovered, and as the symptoms are so very changeable, it must be admitted that the prognosis even here is to be governed entirely by the state of the system.

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IN order to make a more complete analogy between this disease and fever in general, it will be proper to examine for a few moments only, some of the different theories of this disease. And first, that which places it in the

## LYMPHATIC SYSTEM.

THE discovery of the Lymphatics opened a wide field for speculation in medicine; and of all the diseases incident to man, no one has been so constantly ranked in, or implicitly believed really to be of, that system, than that under our present consideration; it of course therefore will be proper first to enquire in what manner this is supposed to take place.

FROM dissections exhibiting the effused water in two forms, viz. in cysts and diffused; it was sup-

posed to be the consequence either of a dilatation or rupture of these vessels. That the former of these cannot be the case, is adduced from the observations of Dr. Monro, who asserts\* that there has not been a single instance in which this dilatation followed the known course of these vessels. It is now however well established that when these are affected by a ptyalism or the venereal disease, their course may easily be followed, in consequence of their enlargement. But these cysts have been discovered the size of an orange, which is as large, according to Monro† as the greatest known aorta without a rupture, and which in its natural size was not only three hundred times bigger than any lymphatic, but whose coats were as proportionably strong. Surely then no one can pretend to suppose these delicate vessels would remain unruptured when so preternaturally distended. But these vesicles have been discovered in the abdomen entirely unconnected with any thing, only floating in the effused liquor. If these were dilated sections of these divided vessels, might not the spot be detected from whence they were detached? But even admitting that a rupture does take place, which no one can believe, it will be enquired, what are the causes of this sudden dilatation; and if this is said to be a stimulus, it must be recollected that the action of this is primarily upon the arterial system.

\* Monro on the Dropsy, page 20.

† Page 21.

THE rupture of these vessels has for a length of time been esteemed a fruitful source of this disease; indeed the fatal experience of many authors proves most incontrovertibly that in it a rupture does sometimes exist: But that this should therefore be imagined its origin or immediate cause, ought not, in my opinion, to follow; for there are many proofs that in most cases of this disease a rupture never was discovered; and there are also numberless instances of these vessels being ruptured and divided, in which no such consequence as dropsy ever was noticed. This, Lower assures\* us, he has often seen, and the experience of every physician must have witnessed the same. But that a rupture is not a very frequent attendant, and of course cannot often be a cause, Monro, Hewson and Cruickshank all unite in affirming that the muscular fibres of these vessels, though delicate in structure, are notwithstanding very strong. But if a rupture is allowed, it must next be enquired, what are the names of those boasted remedies which to effect a cure, must act as vulnerary? And if there are not any, it must be concluded that a rupture cannot be a cause; for with it the operation for the Paracentesis can never promise success, it being only evacuating one quantity of water to make room for another further and greater discharge.

IN Dr. Withering's remarks upon the Dropsy† "he suspects that many of them originate from pa-

\* Cap. ii. page 29.

† Recorded in the Medical Com. vol. v. page 374.

ralytical affections of the lymphatic absorbents ;”  
 and from which possibly Dr. Darwin in the first  
 volume of his *Zoonomia*, has taken the theory,  
 which he has so strenuously attempted to support.  
 But after all the exertions of his great genius, I  
 must be permitted to differ from him in opinion,  
 and that too in consequence of his plausible theory  
 not standing the test of an examination. By a pa-  
 ralysis is always meant a loss of voluntary motion  
 in any part in consequence of either a compression  
 of the brain or origin of the nerves, or upon some  
 other part preventing the proper motion of the  
 said nervous fluid. But says Dr. Darwin\* “a pa-  
 ralysis has been used to express the loss of volunta-  
 ry motion; but may with equal propriety be ap-  
 plied to express the disobedience of the muscular  
 fibres to the other kinds of stimulus.” It must be  
 evident to every one that his definition cannot be  
 admitted; for it is now a very familiar circum-  
 stance to notice muscular fibres under such violent  
 action as not only to be disobedient to other stimu-  
 lants, but entirely insensible to all, and yet not la-  
 bour under a paralysis: this is seen after severe ex-  
 ercise, as in fatigue: also in violent spasmodic con-  
 strictions of the muscles and intestines, and surely  
 in neither of these can it be advanced that a paraly-  
 sis has the slightest tendency to take place.

AGAIN, when the cause of this said paralysis is  
 noticed, it must at once be confessed, that such a

\* Sect. 28, page 297, vol. i.

thing, in all human probability does not occur. This, Dr. Darwin assures us\* is a violent stimulus. It is true, that when a stimulus is received into the system it must be conveyed through the absorbents; but that its effect must of course be upon them is not either from demonstration or reason the consequence: for dissections have never warranted this conclusion, and reason would lead us directly to the contrary: for when it is recollected that its passage must not only be short, but its motion must be in us very rapid, being, according to Cruickshank† in a dog twenty feet a minute, it cannot but be acknowledged that its effects ought not to be sought for in this system. For if it is borne in mind how soon it is emptied into the blood vessels, how constantly it circulates through every one, how absolutely necessary this is both for the support of life and the proper action of even the absorbent system itself, and how invariably its effects are discovered primarily in the blood vessels; it must be granted, that a paralysis of the absorbents in the dropsy is hypothetical, and that this disease is the effect of stimulus acting primarily upon the arterial system.

ANOTHER, and as yet more prevalent opinion is, that a diminished absorption, and that too from a loss of tone in the absorbents, is an immediate cause

\* Page 297.

Page 38, chap. v.

of this disease. That these vessels are at times not able to take up all the effused fluids, and have, as advanced by many, even remained for months in them, being as it were insensible to their action, cannot be denied: But that this should depend upon an abstraction or want of tone from the commencement, does not follow; for there are not any evidences of it, assertions only having been advanced, and all the attributed causes tending rather to increase than to diminish or weaken it. Obstructions in them and their glands, have been by Dr. Cullen and others, supposed of the number. I cannot however suppose that these produce the disease, for it must appear from dissections that it has often occurred without the least symptoms of them, and that they often exist without producing this disease; for Dr. Cullen, although a great friend and advocate for this opinion, candidly acknowledged\* that he had seen several instances of the most part of the mesenteric glands being considerably tumified, without interrupting the transmission of fluids to the blood vessels, or occasioning a drop-sy, and if so, they certainly cannot properly be a cause of the disease. For if they do occur, must they not be the consequence of, either an increased velocity of the fluid in them, or of the vessels appropriated for their nourishment; else of the increased action of the muscular fibres of both sets of vessels? If either of these be admitted, then it must follow,

\* Page 274, vol. iv.

that some stimulus a tergo exists, and on which this increased action depends ; and when that is granted, it cannot any longer be urged that they labour under a loss of tone or direct weakness, for it must be positive that their tone by this stimulus cannot but be increased. But to place it beyond a possibility of doubt, that this disease is not the consequence of a loss of tone in the absorbent system, an extract from the manuscript notes taken in 1786, of the clinical lectures of the celebrated Mr. Cline of London, and obligingly presented for perusal by my friend and fellow-graduate, Mr. Stock, may not be amiss. In lecture fourth, when speaking of the diseases of the absorbents, Mr. Cline thinks “ their debilitated state seldom gives rise to dropsy ; for in that disease they generally appear *larger*, and convey a greater quantity than in health.” With such proof I shall not hesitate to assert, that in this disease these vessels do not labour primarily under a loss of tone, nor are even affected, and that when they are diseased it is in consequence of indirect debility arising from their excessive action. This at once solves the difficulty of most authors, why the absorbents remain in a body of fluid for months apparently inactive ; for only remove a part of this water, or reduce the quantity of stimulus, by any means ; you will take off a part of the load under which they laboured, and thereby they will renew their natural and healthy action, and certainly do their part in removing the remaining effusion : and that this is really the case the remedies which

effect a cure seem to prove; for they are not those which grace the list of tonics, but simply evacuants, as emetics, cathartics, ven. section, and the operation for the paracentesis. From what has been advanced, I presume it is tolerably well established that dropsy does not in common arise in any of the different ways just investigated; and if not, there certainly cannot be the smallest shadow of probability for supposing this disease the continual offspring of a disease of the absorbent or lymphatic system. But when I consider every circumstance carefully, and examine every phenomenon impartially, I cannot even hesitate to decide at once with Dr. Rush and Mr. Cline of London, that “dropsy seems rather to arise from a greater quantity being poured forth by the arteries, than from a defect in the absorbents.”\*

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### LAXITY OF THE FIBRES.

THE impossibility of accounting for many phenomena of this disease, if of the lymphatic system, induced Dr. Donald Monro to advance the laxity of the fibres as the most frequent cause; and which he says† may be produced by great evacuations, a sedentary life, or watery diet. It must here be evident from these causes, that Dr. Monro did not li-

\* Cline's Lecture IV.

† Page 9.

mit his laxity to any particular part of the body, but had a constant reference to the whole system; and as no one will deny, but at times such an apparent laxity does exist, it will be proper to examine its nature before it can be determined if it be the immediate cause of this disease.—By a laxity of the fibres is meant that general relaxation or want of a healthy contraction, the entire effect of an abstraction of a stimulus which was necessary for the health of the person; and which, when again renewed, produces that state. Famine produces a laxity of the fibres, in consequence of food, a natural and healthy stimulus, being withheld; afford this in its proper quantity and it will produce a similar state of contraction or health. The strings of a violin when unscrewed labour under a state of laxity, because the stimulus of the screws for their sonorous or proper state of tension is abstracted; apply this stimulus however again in its particular quantity, and their healthy state or proper tone can be perceived:—precisely such should it be with the fibres of the body. But that this is the state of the fibres, and the immediate cause of this disease, do not appear; for if the causes were able to produce it, they should do it when applied in the healthy state, for only in that situation can they be said to produce a laxity of the fibres. A sedentary life has appeared to be a fruitful source of this disease, but how far it is in itself to, is not certain, for it seems that many people

have for years endured it, without having once exhibited the least symptom of this disease. Now were this an immediate cause, why should it not have produced it in these many instances, and when too a laxity might be expected? for by their action they would reduce the system below the healthy point, and thus have produced it. But say some, it has been found to happen. No one will deny this; but never while the system was in that state, nor before stimulants were improperly used to give, as it has been frequently called strength. For example, let a person who from a sedentary occupation takes but very little or no exercise, call in the use of ardent spirits as a substitute for it, and he will in a short time find himself dropsical. Whereas, had he taken less aliment, and laid aside those baneful liquids, he would, even with the same exercise, have enjoyed his perfect health. This may be accounted for in the following manner: in the former, he enjoyed tolerable health, though his exercise was trifling; but supposing a want of that proper quantity previously accustomed to might affect his health, he hopes to obviate it by the use of more aliment and ardent spirits; when in fact he produces the very evil he wished to avoid; for these stimulants created an inequality of excitement and excitability, and at once raised the system from the healthy to the effusive point. But in the latter, the little exercise created an increased excitability, upon which the moderate stimuli acted, according to a law of the animal economy, with an increased force, and kept

up that equilibrium of excitement and excitability which constitutes health. This familiar example at once shews the impropriety of imagining that this mode of living creates such a laxity of the fibres as is generally believed; and that an effusion is not the effect of such a laxity, but rather of that morbid excitement of the fibres of the blood vessels, the entire consequence of stimuli disproportioned to the excitability of the system. The other said causes act in the same manner, as will be more properly noticed hereafter.

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### INCREASED EFFUSION.

**SENSIBLE** as the late great professor Dr. Cullen must have been of the insufficiency of either of these theories, when separate, to solve the many difficulties arising therefrom, he ingeniously formed\* a compound, which he thought capable of satisfying all objections, and of unlocking many of the mysteries of this disease. An increased effusion or diminished absorption being delivered by so able a man, for a while carried complete conviction to the minds of all enquirers, and obliged those who preferred the sentiments of others to the humble exercise of their own judgments, at once implicitly to receive it as a specimen of human perfection. As the latter part of the Doctor's theory has al-

\* Page 250, vol. i.

ready been discussed, it will be proper here to examine if an increased effusion does take place in the manner by him supposed.

It has been argued by Dr. Cullen\* that an interruption to the free return of the venous blood from the extreme vessels of the body, to the right ventricle of the heart, is a cause of this effusion. That this indeed very frequently accompanies this disease no one can doubt; but that it should be supposed its cause of course, is not altogether so rational; for with equal propriety might it be urged, that as the cold stage preceded the hot, or even existed in the same paroxysm with it, it must be its cause; whereas all admit that the cause of the cold or first stage, is the cause of all that follow. Precisely such is it with obstruction, and that it is not the proper cause of this disease is probable from its being itself the effect of a diseased action of the system; being never produced until the arterial system has previously laboured under a disease. Marsh effluvia, ardent spirits, or fatigue, are proper causes of this disease, because they all act upon the body when in an healthy state, that is, when the excitability and excitement are equal, and must of course, either from an excess of quantity or an improper application of their stimulus, produce this disease; but an interruption to the free course cannot, except from mechanical pressure, be a cause, for it is not a stimulus acting upon the body free from dis-

\* Page 252, vol. iv.

ease, but is itself the effect of an irregular action of the blood vessels, produced by a disproportioned stimulus. Neither does the discovery of a polypus, or an ossification of the valves of the heart, Dr. Cullen's supposed causes of this interrupted circulation, invalidate my opinion in the least; for if an improper use of these stimulants have, from an excessive action of the vessels, produced an effusion of lymph in pleurisy and croup, a rupture of blood vessels in apoplexy, chalk stones in gout, an ossification of the longitudinal sinus\* in mania, and alter, according to Dr. Rush,† even the hair in drunkards: why may it not produce in the vena cava, and heart itself, such morbid phenomena? And further that this is the case appears probable from a fact related by Hewson on the lymphatics‡, and extracted from the minutes of Sir John Pringle, of a man who after having laboured under palpitations of the heart for some time, died apoplectic. Upon dissection the heart was found large and adhering to the pericardium, and had marks of inflammation. In the pericardium was a small quantity of bloody serum; the coronary arteries were ossified, and in the

\* Case in the Hospital, in 1796.

† Dr. Rush in his lectures, informs us, that ardent spirits change the tendons, arteries, and even the pleura in bony matter; and also affects the hair very much, making it crisp: and that the barbers in London are so well acquainted with the fact, that they will not give more than half price for the hair of drunkards.

‡ Page 113.

pulmonary artery and right ventricle of the heart, was a large concretion, supposed to be a polypus; the upper part was white and fizy, and the under like congealed blood. If such then be the appearance from dissection, and from which no dropsy was the consequence; is there not a sufficient reason to conclude that this interruption of the blood from the extremities to the heart is not the cause of this disease, but is, like a polypus and ossification of the ventricles of the heart, the entire effect of a morbid state of the blood vessels?

Not only an interruption to the course of the blood through the heart and large vessels, but also obstructions of the different viscera have been by some imagined a frequent cause. A similar objection to these might be advanced; for in all there had pre-existed a morbid action of the blood vessels, for in ninety-nine of an hundred cases, they are the consequence either of an obstinate, or of an ill cured fever, which is now universally acknowledged, a disease of the arterial system. Dr. Cullen\* appears to have been perfectly satisfied that an obstruction of the liver was a very common cause; and this he supposed very probable from the situation of the vena portarum, being such that this obstruction could not but impede the regular return of the blood. No dissection however has as yet confirmed this plausible opinion; for it has on the contrary been discovered that these vessels are not only

\* Page 254, vol. iv.

undiminished in diameter, but much enlarged: accordingly, it may be imagined that they convey more blood than natural. This is not hypothesis alone, for frequently this viscus has been found schirrous:—now this state is certainly the consequence of inflammation; and this by all is acknowledged the effect of a morbid action of the blood vessels. But that obstructions are not a frequent cause of this disease, is evident from the writings of a late German author, Dr. Richter, who says\* he has often found ascites and hydrothorax without the smallest observable fault either of the contents of the thorax, or abdomen; and on the contrary has often found such morbid appearances in the highest degree, without the smallest tendency to this disease. And if all this be granted, I must conclude with the Doctor, that as this disease disappears for a time and again returns; is sometimes present in a slight, and sometimes in an alarming degree, accompanied with different symptoms at different times; it must be incredible that obstructions in the viscera, a cause which continues constantly to act, could so vary and change in its effects.

HAVING, I hope satisfactorily proved, that obstructions of the viscera, are not a frequent cause of dropsy; it is now proper to assert that they are not so, as they naturally happen, in the human system at any time; being always themselves the effect of

\* Page 235.

a morbid action of the blood vessels, as much as supuration, rupture, or gangrene. But when they do occur, they may from the stimulus of distension greatly hasten the already turbulent motion of the vessels. An experiment or two taken from Lower and recorded by Dr. Wilkes and others, sufficiently prove this. "If the jugulars of a dog be tied so as to prevent the blood's circulation, he will shed an abundance of tears, the saliva will flow as fast from him, as if a ptyalisin by mercury was raised, and a serous matter will ooze out betwixt the integuments of the head and the interstices of the muscles of the neck. Again, if the vena cava be tied a little above the diaphragm, the abdomen will fill with water, as in an ascites." These experiments at once prove that effusion is the effect of an increased action of the blood vessels, and that obstructions may increase this action. The obstruction here however is very different from those which take place in this disease; and that too in being mechanical, and in acting upon a healthy system, and producing from their indirect stimulus, a disease of excessive action of those particular vessels; whereas the others are only the consequence of a primary disease of these vessels, and whose action of course does not produce a disease in them; there being an axiom in medicine which assures us, that two diseases cannot exist in the same system at the same time. Again, that obstructions are not an immediate cause of this disease, is argued from the cure; in order to effect which such medicines are prescribed, whose known

action is evacuant, and chiefly upon the blood vessels ; and obstructions as a cause are entirely lost sight of, and are only noticed as symptoms, the consequent attendants of a morbid state of the blood vessels.

DR. CULLEN says\* of all the causes of an increased effusion, the most frequent and remarkable is, a laxity of the exhalants ; and since whom it has been so universally admitted and believed, that it has been the bulwark of all who wished to defend his theory ; the compass of those who wished to steer through this disease ; and also the stumbling block of all who have failed in its cure. As a laxity of the fibres in general has already been noticed, it will only here be necessary to enquire into the cause which produces this laxity of the exhalants in particular ; and which is most frequently a general debility of the whole system. Much has indeed been said by most authors respecting this debility ; but from all it must be evident, that little else than a repetition of opinions, without a proper investigation, has been advanced ; and that it has never yet been proved that in any case a simple direct debility has ever produced this disease ; for Dr. Rush has assured us in his lectures, from experience that it never took place in those states or conditions of the system in which direct debility occurs in the highest degree ; for says he, 1st. “ in old age this state of the system is universal, and yet how seldom do any old

\* Page 258.

people die of dropfies ! Nay, how feldom do we obferve even a fwelling of the feet to take place in old people, who fit for months and years constantly in arm chairs by their fire fides ! 2d. In the laft ftage of typhus or low chronic fever, dropfical fwellings are unknown, where previous evacuations have been ufed, or when they have been unneceffary. 3d. Dropfical fwellings feldom occur in perfons who die of pulmonary confumption. 4th. They never occur in cafes of marafmus and atrophy, in which patients die at laft from fimple debility. 5th. And laftly there is not a fingle inftance of a dropfy in thofe perfons who have fuffered or died of famine ; now in death from this caufe, direct debility is always in the higheft degree.” But what are the caufes which can create this debility of the exhalants without affecting the pulfe, which is generally in this difeafe frequent, quick and tenfe, and the blood when drawn becomes fizy ? Or why are not the contractile fibres of the arteries relaxed by the fame caufes ? If they are, then the circulation of the blood muft be diminifhed in an exact ratio to that of the exhalants, and of courfe prevent this great and fudden effufion. Fevers of all kinds, and intoxicating liquors are faid by Dr. Cullen\* to be the moft frequent caufes of dropfy, and thefe produce the difeafe not from their debilitating effects, but their ftimulating, as muft be fenfible to all who feel the pulfe during their action. If therefore, as has been proved, this difeafe is not the confequence of a

\* Page 259, vol. iv.

debility of the exhalants, and if the causes said to produce this laxity of them, tend rather to excite their action; may it not be fairly inferred, that this effusion is not produced in the manner just investigated? And that this debility, which Dr. Cullen calls the *hydropic* diathesis, may with equal propriety be changed to the *febrific*, being as constant in the one as in the other?

THIS increased effusion has likewise been attributed to a larger quantity of water than usual being in the system. It will therefore be necessary to turn the attention to its numerous sources. It has been supposed the consequence of the excretory vessels not performing their office. That this is a frequent cause of this disease does not appear probable, for the excretories have been affected in the other states of fever, when no such thing appeared in the blood as a greater quantity of water, and no dropfy was the consequence, and frequently a dropfy has been when the excretories were regular, and when the blood exhibited the same appearances as in an inflammatory disease; and if so, this can seldom or never be a cause.

THE quantity of water has been said to be increased by blood letting. Never has an opinion been more implicitly believed, and more inviolably held sacred; and yet when it is examined, it must with astonishment be confessed, that this opinion, only because of ancient date, has led men,

called rational creatures, so easily astray. It will therefore be necessary to determine if such a thing takes place : and this is generally imagined to be done by its diminishing the crassamentum ; but whoever has once seen blood drawn must acknowledge, that from this apparent homogeneous mass an equal proportion of serum, red globules, or coagulating lymph to their respective quantities in the blood, must be taken away, and therefore their exact ratio must be the same after as before a bleeding. For instance, if three kinds of fluids in different quantities are intimately mixed and kept so by a kind of constant circulation, it must be evident to all, that if a pint of this mixture be evacuated during its course, the remaining quantity will not exist in a greater disproportion than was before the discharge. Precisely so is it in blood letting. Dr. Sydenham was an implicit believer in the blood being thinner and weaker in this disease, so great was his faith in the opinion of others ; and this is truly surprising, for he was the first who taught us that in diseases of severe and inflammatory action, the particles are so broken down from the excessive action of their blood vessels as not to unite, and of course appear in that form. But say some, after ven. sect. the absorbents may take up the deposition in the cellular membrane, and convey it into the blood. Although this undeniably happens, yet it does not follow that the blood is necessarily more watery ; for it is not certain but that as soon as it enters the blood vessels it there may undergo such a change as

to be converted into the proper proportion of the constituent divisions of the blood. And this is certainly probable ; for if not, after several bleedings the blood would seem to be mostly water ; but on the contrary, in some states of fever, after ten or eleven bleedings, so far from there being much ferum there has not apparently been any at all, the blood being dissolved.

FROM what has been advanced it may fairly be doubted whether an excessive evacuation, from ven. section ever produced a dropfy ; for there are innumerable instances of this disease without this evacuation being once used, and a number of almost incredible losses of blood without inducing this disease. Dr. Rush has taken repeatedly by 13, 14, 15 bleedings from 100 to 140 ounces, without producing this disease. Dr. Physick has taken 85 ounces by one bleeding without inducing one of its symptoms. Dr. Dover, an English physician, in 1732 bled 180 soldiers about 100 ounces at a time, and lost only 8, in whom he could not prevent the use of ardent spirits. Botallus not only drew large quantities of blood without any consequent dropfies, but always bled in them. And Haller\* records a number of cases both in frequency and quantity without the least injury or ill consequence. The two following are the most remarkable. In the first, in twenty-four hours 202 pounds of blood was lost. In the second, a lady was bled 1020 times in

\* *Elementa Physiologiæ*, tom. ii.

19 years. If upon the whole, such large quantities of blood have been drawn without causing this increased quantity of water in the blood, or even creating a laxity of the exhalants, and thus have produced a dropsy, may I not be allowed to conclude, that this disease is not the production of that invaluable remedy? But some may perhaps urge, that although a variety of opinions may exist, yet experience has taught that this disease has sometimes come on after these evacuations. This I do not deny; but it should not be attributed to its excessive or injurious application, but rather to its *too sparing use*. For œdematous ancles have often been seen after 2 or 3 bleedings, which had entirely vanished upon a repetition of the use of the lancet, and health has taken their place; from which it must appear, that although swelled ancles have been the polar star of some physicians, and have at once convinced many others of the inevitable consequence of the lancet, a DROPSY, yet it is in itself an inestimable remedy, producing it only in the hands of the timorous; and swellings are only a symptom of a morbid action of the blood vessels, and are in themselves as trifling and innocent as a pain in the side or breast, and always *calling* for a repetition of the remedy. For when physicians in common pretend to open a vein, the system must certainly be raised much above the effusive point; but by 2 or 3 bleedings it is reduced from that higher or inflammatory point down to the effusive. Not knowing this, they at once sheathed the lancet, and

the disease by continuing at this point, produced the disease in question : whereas had they used the lancet a little oftener, they would have reduced the system from the effusive point to the simple and less dangerous point of morbid action alone, and thereby the former disease would have been cured without either difficulty, a dropsy, or even a stigma to the lancet.

It has been urged, that a watery diet may produce a thinness of the blood. This, however, as yet is only an hypothesis, for Dr. Millman\* has ably defended the use of a watery diet in this disease, and assured us that the Italian physicians threw in large quantities of water in fevers : nor did they fear a dropsy from what they call their *dieta aquea* ! And it is well known, that many dropsies have existed without such a diet, and in all whom the blood never exhibited such a watery appearance. Besides, even allowing a watery diet to be used, as it is not known what part of our aliment forms the red globules, and what the lymph ; and as the chyle appears much the same although the production of a variety of aliments, the complete formation of the constituent parts of the blood must depend not upon the quantity of water, but upon the *appropriate* action of the blood vessels.

THE different methods of an increased effusion have now been noticed, after which I hope it will

\* Page 22.

be confessed that it is not probable that it happens in either of the ways advanced by Dr. Cullen. But notwithstanding which I am far from *denying that this effusion never exists*; for I hold, that it always does, and is properly the only manner in which this disease can take place; and is always the effect of a morbid action of the blood vessels, produced by the application of an over dose of stimulus, or otherwise, a stimulus disproportioned to the excitability or state of the system.—And this is more evident from their

## C U R E.

IT has constantly been an object with authors in medicine, to make their cure coincide with their different theories: but as the experience of ages has confirmed the propriety of truth being an unit, in the case of no one disease has it ever been more fully exemplified than in the present. For notwithstanding a variety of different theories and modes of cure have existed, yet the trial of them all has been so uniformly fatal, that even in our time, this disease is by many ranked as *incurable*. When a defect of the absorbents was in vogue, it was found that those medicines which ought to have promised a cure were prejudicial; and those which fortunately were serviceable, were strangely accounted for: for example, emetics have often in the annals of medicine been recorded a means of cure; and instead of attributing this to their proper evacuating

quality, they were said to *promote* the *absorption*; and sometimes they have produced a perspiration, and evacuated greatly thereby; then they were said to *overcome* the *spasm* by tending to the *superficies*. Debility, whether of the fibres of the body or of the exhalants, or in whatever manner produced, has at all times been admitted as the most general cause of this disease; and yet the first indication here has been to evacuate the water, by medicines whose action must have debilitated still more, as cathartics, which may be seen in Drs. Cullen and Sydenham: when in fact, according to their own theory, their first indication ought to have been to strengthen these debilitated fibres, by tonics; and when the system had become stronger, then to discharge this water. But how could Sydenham himself have prescribed purgatives, and that too of the most drastic kind, in a disease of such debility as he supposed this to be? And yet he assures us, these promised success. While this disease is treated as of debility, if a glance be only cast at the many sufferers who daily fall victims to it, it must be evident, that not even the yellow fever itself has made greater devastation. If so, the solitary instances of cure which these medicines have effected, must be accounted for very differently: for if this disease be only admitted, a morbid action of the blood vessels, the operation of all these remedies can at once be made plain; for it is certain that purges act by evacuating or depleting from the mass of fluids in the body, and thereby diminish this

morbid action of the blood vessels: that this is the case in the different states of fever, all must acknowledge; and precisely such is their effect in this disease. Emetics have not only the power of evacuating the contents of the stomach upwards, but also of discharging the feces copiously downwards, and promoting the perspiration freely, by which it cannot be denied but a large evacuation takes place. And thus likewise is the effect of blisters, diuritics, and a salivation; for all of which are undeniably evacuates, only differing in force, and of course, whose application must always rest upon the state of the system or the will of the physician.

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AS it has been fully proved by the symptoms, causes, and method of cure, that this disease depends upon a morbid action of the blood vessels; it must be acknowledged to be as much a FEVER as any other disease; and that this is really the case, its similarity with the other states of fever will more completely establish.—And

I. Are fevers most commonly the production of *exhalation* from low marshy countries? So likewise is the dropy. Towne on the Diseases of Barbadoes\* observes, that before the island was rendered less moist by cultivation, this disorder was so endemic as to be distinguished by the title of the

\* Page 11.

*country distemper.* Wilkes says\* this disease is very common in fenny places, and is owing to the atmosphere being overloaded with noxious vapours.

And Dr. Richter† asks, if experience does not teach us that dropfy is much more frequent in damp marshy countries, and there more difficult to cure than in dry ones: and that in warm climates this disease frequently disappears of itself. In point he relates the case of “an Italian merchant who sat at an ordinary in Holland, eating with a good appetite, and was all over dropfical. Upon enquiring into the state of his health, and advising him not to neglect his disease, was answered, that he was convinced his disease was of but little consequence, for he had already been five times in Holland, and had at each time got the dropfy, which had always gone off, as soon as he had arrived at his home in Italy.” Most authors have noticed the frequency of this disease in moist cold weather, and in wet situations. That this is the effect of marsh effluvia no one will deny; for it has on all hands been allowed to be its most fruitful source. How this acts, whether as a sedative or a stimulant, or whether it is absorbed by the pores of the skin, or received into the lungs, and there undergo a change, has for a length of time been disputed. That it is a stimulant, I take for granted, in consequence of its producing a fever, which I hold to

\* Page 320.

† Page 291.

be the effect of an irregular or morbid action of the blood vessels, and produced by a stimulus: and because the Professor of the Institutes assures us, its action is direct, stimulating the blood vessels, and creating many diseases of an inflammatory nature, as fevers of most kinds, as the bilious, remitting, and dysenteric. As to an absorption, I shall not here pretend to say but that such a thing may possibly happen; yet I cannot imagine it probable that this disease is produced thereby, for the body is most commonly so well clad as to prevent it in such a degree as must be requisite to create it; and because there are not any proofs of such a thing taking place in any of those who have had this disease. And more especially, because it can be more easily accounted for by imagining the lungs to be the channel through which exhalation is received to produce its effects upon the system. Although I do not pretend to decide in what constituent part of the exhalation its injurious quality may reside, yet may it not, be it in what it will, be in a greater proportion in a given quantity of some states of the atmosphere, than in others, and of course may be taken in the lungs in a larger dose, and thus raise the state of the system, by its stimulating property, from the *healthy* to the *effusive* point, and produce this disease? And this seems probable, for Chaptal\* asserts, that vital air in respiration passes continually from the gaseous to the concrete state;

\* In his Chemistry, vol. i. page 137.

it must therefore at each instant abandon the heat which held it in solution, and in the state of gas." Shortly after he says, "hence it follows that during the winter, the heat produced must be more considerable, because the air is more condensed and exhibits more vital air under the same volume."— In like manner exhalation in consequence of the abundance of caloric in dry and hot weather, must be kept diffused, like vital air, in a larger volume of the atmosphere, and must be in a more complete gaseous state; and if so, must be breathed in a less quantity at every act of inspiration, and of course must produce a less injurious effect than when the atmosphere is colder, more moist, and condensed. And that this is really so, the following fact cannot but prove. Dr. Rush informed me that a gentleman in Maryland about to return home in the evening from a neighbouring visit, regretted that he had to pass by a place which always about that time sent forth a very disagreeable foetor. His friend pressed him to remain until midnight, when he assured him, this offensive smell would not be perceived; he did so, and the event was precisely as predicted. This important fact in itself evinces the propriety of this opinion; for the action of the sun being withdrawn, the caloric became more latent, and the atmosphere was so condensed as to concentrate the exhalation in that portion of the air which was within his height, and which of course acted so forcibly upon the organ of smell, and which gave such uneasiness in respiration; but

at midnight the atmosphere was not only concentrated as much as possible, but the exhalation must have been precipitated to the earth, and thus have left the surrounding air perfectly sweet. If all this be acknowledged it must be confessed that EXHALATION acts in this disease as in other states of fever, and in all it produces a morbid excitement of the blood vessels. This indeed seems to be the case, for this complaint is most prevalent in wet situations, intermittents in those less so, dysenteries in those the least so, and when neither of these exist, and where the sun and air have a free access, diseases cease to be an evil to mankind. Some may perhaps object to this mode of explanation, because in some countries where as much water falls in one year, as in any one place, this disease is scarcely known. This I can easily conceive may be the case, and yet not prove a powerful objection; for Dr. Rush in his lectures has very properly taught us, that the action of marsh effluvia in a great measure depends upon the situation of the ground, and its moisture. If the land be level and completely overflowed, the exhalation is trifling and not injurious, and thus he says Mr. Bruce in his travels remarked that the rainy seasons, which perfectly covered the low grounds, were never unhealthy, in one of the sickly countries which he visited. But in some others which have no morasses, and are more hilly, Daziller observed\* the rainy seasons to

\* In account of Dis. Neg.

be the reverse. And that a great fall of rain may happen in a place and not produce this disease, is very probable; for I can easily believe that so great a quantity may at once fall as not only to check, but even prevent for a time any exhalation. This in a short space of time however must cease to act, and would be the very instrument of its generation if a succeeding shower or two, did not so diminish it as to prevent that quantity to be generated at any one time as is requisite to create febrile diseases. This is generally observed in our common epidemics, which are always prevented from being fatal by succeeding and refreshing showers. If this proof is not sufficient, I trust the following, from Huxham†, will place it beyond a doubt. In August 1735, the month was so remarkably wet, that every place was filled with mud and water. The birds died in moulting. The leaves of the trees fell as if it had been in the midst of winter. And in all this month not a single dropfy is mentioned by Huxham as one of the diseases. In September the weather continued just the same; and among its diseases there was not the least vestige of a dropfy. But in October the season was not so wet, and in this month intermitting and *asthmatic* fevers, as he called them, terminated in a dropfy of the abdomen and feet. This is now in itself so evident that I shall at once decide that a dropfy is as much a fever as the intermittent or other states; and that in ALL, this exhalation is the remote cause, producing

\* On Epidemics.

by its stimulus an irregular or convulsed<sup>ive</sup> action of the blood vessels.

II. Do the other states of fever occur at the same season? In like manner does the hydropic state. That the different states of fever do occur at the same time, must be familiar to every one; and that the dropsy frequently exists at the same period with other diseases of the arterial system, all physicians and authors of experience have admitted. Dr. Richter\* says, in the autumn of 1791, when almost all diseases were of the rheumatic kind, dropical symptoms accompanied them. Huxham says, in 1747 there was a putrid fever which frequently was attended with a swelling of the abdomen. And that it should exist at the time of a rheumatism, or pneumonia, is indeed now not very surprising; for it has been, I hope, incontrovertibly proved to be of the same system, produced by similar causes, which create the same irregular action of the blood vessels.

III. Have all the other states of fever debility for their predisposition? Such also is the case in the hydropic. The term debility has most undoubtedly slain its thousands; for while all have admitted it as an object in the cure of diseases, few or none have understood its nature; most of them, ranking, as of that order, all diseases in which the patient either looks *pale* or feels *weak*, or the *na-*

ture of which they do not comprehend. And never before the time of Dr. Brown, were it taught to be of two kinds, *direct* and *indirect*. This distinction when kept in view, cannot for its usefulness, but reflect the highest honour upon its author; yet unfortunately, he himself lost sight of it, when he assured us, that although arising from either an abstraction or excess of stimulus its cure must be effected in the same manner. This inconsistency and contradiction however must become harmless, when it is recollected, that the *debility* arising from *intoxication* and *famine* must be remedied very differently. The Professor of the Institutes has in his lectures not only taught the propriety of this distinction, but in the IV. Vol. of his Enquiries has most ably and successfully proved them to be in themselves not a disease, but only a predisposing or inviting cause. And that it is precisely such in dropsey, the following fact, which came within my own knowledge, will sufficiently testify. A gentleman was suddenly reduced from a common and healthy diet, by confinement, to bread and water; in this situation he remained for months, and enjoyed his health without the least symptom of disease. After this, his regimen was suddenly altered, and a more stimulating and nourishing one allowed; when lo! in three or four days an inveterate dropsey was the consequence, which took three bleedings, in each of which the blood was fizy, and a salivation to subdue it. This instance at once evinces that *debility* simply is not in

itself, a disease, nor the immediate cause of dropfy; but only an inviting cause. For by the abstraction of these stimuli the excitability was so much increased as to enable the moderate ones, of bread and water, to keep the system to the healthy point; but the more nourishing diet immediately raised the system from the healthy to the effusive point: and thus a dropfy was the consequence, which now must undoubtedly be admitted, the *effect* of a morbid action of the blood vessels.

IV. Are particular constitutions more liable to some states of fever than to others? Precisely the same is it with the hydropic fever. Most authors have remarked that men are not so subject to it, nor so difficultly cured, as women; and that children are easier relieved than either. It is not uncommon to observe some families subject to the arthritic, others to the apoplectic, and others the hemorrhoidal states; so likewise Hippocrates\* has noticed this hereditary disposition in the dropfy: And Dr. Wilkes† had a female relation who had been subject to it from her infancy, and who at the age of 50 died of it, and whose mother not only laboured under it when with child of her, but also fell a victim to it. And Huxham, in lamenting the loss of his consort,‡ says “she descended alas, from an hydropic race.” Shall I now hesitate in declaring this disease to be of the same nature as a fever?

\* Lib. prædict. ii. page 89.

† Page 124.

‡ Page \*24.

V. Do the other states of fever attack all ages and both sexes? So also does the dropfy. And that this is really the case, the experience of all practitioners must determine.

VI. Do the different states of fever alternate with each other? Such undoubtedly is the case in the hydropic state. Dr. Rush says\* he has seen anasarca alternate with vertigo, and both ascites and anasarca with tonic madness. Dr. Darwin† mentions two cases of dropfy having a temporary cure from insanity. In the rheumatism it seldom happens, says Dr. Cullen,‡ that a swelling coming on does not alleviate the pain of the joint: and Dr. Richter§, mentions a violent case of rheumatic head-ach, which suddenly vanished after the feet swelled; in seven days after, the head-ach returned, and the swelling disappeared. Also§ of a dropfy from an ill cured itch, the patient was inoculated with itchy matter, and the disease had scarcely made its appearance, when the dropfy disappeared. But of the many facts related by that elegant author, the two following are the most important. In the one§ “the patient had first a catarrh, then pains in the knees, which were so violent that he could not move; soon after his abdomen and face swelled; the pains in his arms and breast were increased, until an expectoration of a

\* Vol. ii. page 167.

† Page 15, vol. ii.

‡ Page 295.

† Sect. 34. page 435.

§ Page 291.

¶ Page 313.

purulent colour, which was sometimes streaked with blood, took place: after this his dropfy abated, and he was now violently attacked with a stitch in his breast, with a full, quick, and tense pulse. The dropfy now quite gone, but the pains more violent, sometimes in the knees, and sometimes in the arms, attended with a fever. The swellings of the legs returned, which gave some relief to his pains. These swellings were now attended by a little inflammation, which, however, went off with a copious perspiration, and the patient was shortly after freed from every complaint." In the other,|| "sometimes the neck alone swelled, sometimes the face, sometimes the arm, and sometimes the feet. Twice the patient had all the symptoms of the hydrothorax, and once a dropfy of the lungs. He had ascites repeatedly, and all these swellings changed and took place so suddenly, that in the evening there was not the least appearance of that swelling which in the morning was prodigiously large."— Jackson says,\* dysentery and dropfy frequently made their appearance in the months of August, September, and October, alternating or succeeding the intermittent. And in another place†, he says, "at Camden, South Carolina, the intermittent, the dysentery, and dropfical swellings so often alternated with one another, as evidently shewed that they all

\* Page 197.

† In his Treatise on the Intermitting Fever of North America, page 199 and 200.

‡ Page 198.

depended upon the same general cause." That some states of fever should alternate with others, is no thing remarkable ; but that a dropſy ſhould, has appeared wonderful ; this, however, will no longer be ſo, when it is recollected, that all thoſe diſeaſes which do this are primarily of the ſame ſyſtem ; for no two diſeaſes can exiſt at the ſame time in the ſame ſyſtem, as the ſtimulus producing the one muſt be ſtronger than the other, and of courſe deſtroy the *one morbid* action by creating a *different* and *greater* action. Upon this principle hangs the ſolution why dropſy alternates with theſe other ſtates of fever.—And as an additional proof

VII. Do the other ſtates of fever terminate in each other? So likewise does the hydropic ſtate. Every practitioner muſt have witneſſed this in the other diſeaſes ; and that this is really the caſe in the dropſy, the following facts muſt inevitably prove. It is not uncommon to ſee this diſeaſe terminating in the form of a diarrhœa ; this has been noticed from the time of Hippocrates to the preſent period : as is ſufficiently confirmed in the writings of Hippocrates, Ferriar's Medical Eſſays, and Wilke's Treatiſe on the dropſy. Dr. Cullen\* ſays he has remarked a fatal hydrothorax, and there are many inſtances in which a ſpitting of blood came on ſeveral days before the patient died. Wilkes records\* a caſe which terminated in an epiſtaxis.

\* Page 317. vol. iv.

† Page 48.

Mr. Wiseman says\* he was consulted for a prolapsus ani; upon observing the sheet upon which the person was, much wetted, he concluded he was dropical; the patient died apoplectic, and upon dissection, much water was found in the abdomen. Dr. Cullen† says it is not unusual for a general dropfy to end in apoplexy.—As a further evidence of the sameness of this with the other states of fever

VIII. Are some states of fever the entire consequence of others? And such is truly the case in the hydropic. That this happens in the other states needs no demonstration. Wilkes‡ has known a colic to produce an ascites: and in the Medical Commentaries§ is recorded a similar instance in consequence of a bilious colic, [and which terminated fatally after 928 pints of water had been discharged in 15 months by 25 different operations. In the IV. volume|| is recorded a dropfy from the angina maligna. Dr. Cullen¶ says there are instances of pneumonia terminating in hydrothorax. Also\* that asthma, after a long continuance, often ends in it. And Huxham in 1746 records a severe pleurisy which when ever it ended fatally, death was always foretold by watery swellings of the feet. That dropfy is the consequence of remittent and intermittent states of fever, Lind, Pringle, and most authors affirm. Dr. Richter\* asks, how many exam-

\* Book iii. chap. 2.

† Page 45.

|| Page 75.

\* Page 400 vol. iii.

† Page 145, vol. iii.

§ Vol. iii. page 503.

¶ Page 241, vol. i.

† Page 290.

ples are there of dropſy being the effect of the itch improperly cured ; of eruptions of the ſkin which had gone in, of the venereal poiſon, of the ſcarlet fever, of rheumatism, and of gout ? And if all this be acknowledged, and as moſt of them are known to be diſeaſes of a morbid excitement of the arterial ſyſtem, it muſt alſo be admitted, that the dropſy, as well as all thoſe muſt depend upon a morbid ſtate of the blood veſſels.

IX. Have all the other ſtates of fever two ſtages, acute and chronic ? So alſo has the hydropic. That this is ſo in fevers in general, will undoubtedly be admitted : and that there are alſo two ſtates in the dropſy, no one has ever denied ; this indeed has always been acknowledged. Dr. Ruſh, in his II. volume of Enquiries, not only allowed them, but has called them the *tonic* and *atonic* : the latter to be known by occurring in habits naturally weak. With all deference, I ſhall here prefer the terms *acute* and *chronic* ; and that too, firſt. Becauſe tonic ſuppoſes the ſtate always to have what has been called a phlogiſtic diatheliſis, and muſt be of ſo inflammatory a nature as at all times to require depleting remedies ; which is not the caſe. Whereas the *acute* only aſſures us, the diſeaſe muſt terminate in a ſhort time, as all other diſeaſes of that nature : and though generally of a violent morbid action, it does not induce any one to ſuppoſe it muſt *always* be ſo ; for its morbid action may be ſo ſlight as to be reduced by moderate evacuations, or even to be overcome, as in the other ſtates of fe-

ver, by a tolerable stimulus, which might create in the system a greater and, possibly, a different action.

2. I shall use the term *chronic* because it supposes the disease to be of some standing, by which its morbid excitement must be greatly diminished; and consequently its excitability must be considerably wasted; whereas *atonic* conveys an idea of weakness from the commencement, in which the excitability must be increased. Now this we know not to be the case; for if the excitability were increased a moderate stimulus would have effect in curing this chronic state, and that too because it is an invariable law of the animal economy, that the action of a stimulus will be more powerful as the excitability is increased; but on the contrary, experience daily teaches that in the *chronic*, the most powerful stimulants are requisite, and that too because there is another law which also assures us that to create a new action the stimulus must be greater than that which already occupied the most of the excitability of the system: and this is known to be precisely the case not only in this state of dropsy but in all chronic diseases. Besides, it will often be necessary to reduce the system in the chronic by evacuants, in order to diminish still more the morbid excitement, that these stimulants may operate more certainly and expeditiously.

3. I prefer the term *chronic*, because Dr. Rush has very properly taught us that the weaker the subjects are, not only more liable must they be to diseases of great morbid action, but the

more requisite must evacnants be to rescue the debilitated vessels from rupture.

X. Have some states of fever different kinds of effusion? So also has the hydropic. Every person the least conversant with this disease, will confess that this is the case; for not only serum and blood, but effusions of all colours and consistencies have at different times been discovered: But that is not all, even hair, bone, and teeth have been found upon dissection; instances of which may be seen in Wilkes: and I have been told that Mr. J. Hunter used to exhibit a diseased rectum, in which a tooth had grown. These curious and wonderful phenomena have at all times remained unexplained. Can they not be accounted for by admitting, that as it is now pretty evident, that an effusion is the effect of a disease of the blood vessels, and as the different parts of the body are both formed and nourished by them, and as Dr. Rush has in his IV. volume of Enquiries made a misplaced state of fever, may not where these phenomena happen, not only a *misplaced*, but a *miseffused* action exist in those particular vessels?

XI. In the other states of fever have dissections discovered the same morbid phenomena? Such also has been the case in the hydropic state. It is not by any means a very uncommon circumstance in all the different states of fever to find inflammations, adhesions, and pus, all the entire effect of a violent action of the blood vessels. So likewise in those of dropical subjects are the same appearances. Fer-

riar has given a number of dissections, most of which exhibited such marks. So likewise has Dr. Millman\* noticed such; as is also recorded in the Medical Commentaries†. And Morgagni is overstocked with them; and the many dissections in the Pennsylvania Hospital for a year or two since have confirmed the truth of all former ones, to which I beg leave to refer. Besides, its similarity with the hydrocephalic state of fever which has been proved by Drs. Quin and Rush as of excessive excitement at once shews that it must depend on stimulus acting on the blood vessels, and there producing such an action as to create these same morbid phenomena.

XII. In the other states of fever, does the blood shew a morbid appearance? Such precisely does it in the hydropic state of fever. That this is the case some very respectable authors have witnessed. Dr. Rush says\* it has been noticed by many writers. Dr. Millman† bled with great relief, and found it fizy. Dr. Wilkes not only experienced the truth of this, but even says, if blood could be drawn in this disease, without a hazard of reputation, it would always be found fizy. And Botallus bled in this disease without the least inconvenience; and I dare say with good effect, for I have often seen blood drawn in this disease as fizy as in the severest pleurisy. These instances are enough to convince

\* Page 46.

† Page 503. vol. iii.

‡ Vol. ii.

§ Medical Pamphlets, page 98 & 106.

all, of the UNITY of *dropfy* and *fever*, and are sufficient to establish the propriety of bleeding in this disease, if no others could be adduced. But there are others, for

XIII. In the other states of fever, is the pulse always in a morbid state? Such also is it in the hydropic state. This many physicians have noticed, and all have united in declaring the presence of a morbid action of the blood vessels. Dr. Rush in his lectures assures us, it is full, quick, tense, and sometimes slow, as in the other states of fever. Dr. Millman has also found it full, quick, and sometimes slow. And the experience of all who will examine it, must confirm it to be as in the other states of fever. It is indeed much to be lamented, that physicians have so long neglected the pulse in this disease: this being always superceded by the more prominent abdomen and palid countenance; and which have for ages, been a general and sufficient evidence of a great weakness, and want of blood. And when indeed blood has been taken, it was always in the most sparing manner, and never before an *excrutiating pain* in the side or breast had loudly *called* for it; called I said, for without it, were the blood vessels ready to burst, the lancet would remain sheathed; but with it, even were the morbid action so slight as to be overcome by a stimulant, *bleeding* would not only be fashionable, but might be performed at all times without the least hazard of reputation. Such is

the effect of prescribing for the name of a disease !  
Whereas, was this disease considered as a FEVER,  
as it really is, we would attend not to the appearance of any one particular symptom, nor would we prescribe for the name, but would at all times be governed by the state of the system, which could always be discovered by the pulse.

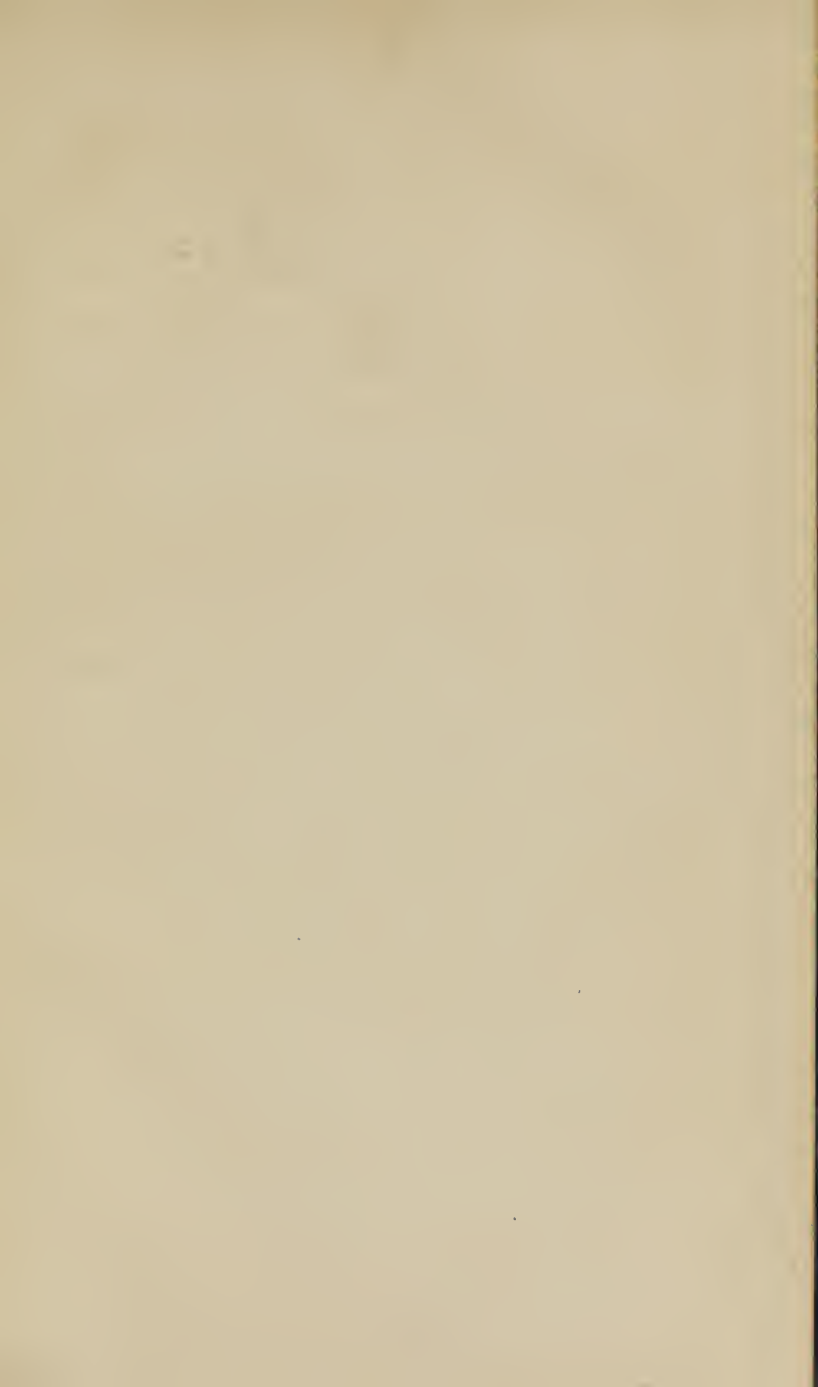
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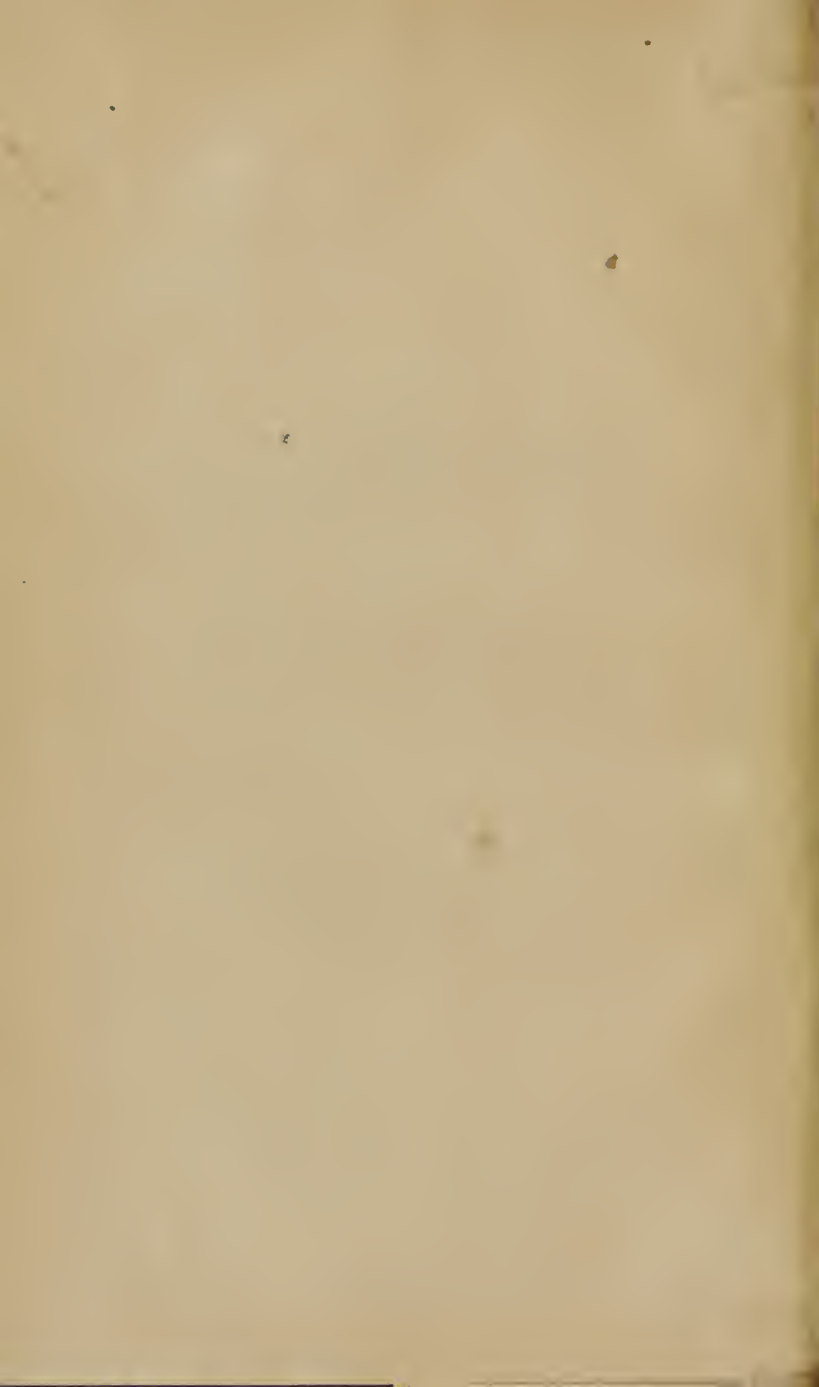
WHOEVER examines impartially what has been advanced in favour of this disease being of the same nature as fever in general, must acknowledge, that as it has been supported, and I hope established, from facts, as arising from the same causes, produced in the same manner, and affecting the same system ; that its

### PROXIMATE CAUSE

must of course be the same, and is an irregular or convulsive action of the arterial system. And if all *this* be admitted, its CURE must undoubtedly be conducted in the same manner ; always bearing in mind, that the remedies are ever to be adapted to the STATE OF THE SYSTEM.

F I N I S.





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